# Letter

# Modeling Spatial Heterogeneity and Historical Persistence: Nazi Concentration Camps and Contemporary Intolerance

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*A wealth of recent research in comparative politics examines how spatial variation in historical conditions shapes modern political outcomes. In an article in the* American Political Science Review, *Homola, Pereira, and Tavits argue that Germans who live nearer to former Nazi concentration camps are more likely to display out-group intolerance. Clarifying the conceptual foundations of posttreatment bias and reviewing the historical record on postwar state creation in Germany, we argue that state-level differences confound the relationship between distance to camps and out-group intolerance. Using publicly available European Values Survey data and electoral results from 2017, we find no consistent evidence that distance to camps is related to contemporary values. Our findings have implications for literatures on historical persistence, causal inference with spatial data, Holocaust studies, and outgroup tolerance.*

In "Legacies of the Third Reich: Concentration Camps and Out-Group Intolerance," Homola, Pereira, and Tavits (2020) (hereafter HPT) argue that individuals who live nearer to former Nazi concentration camps in Germany are more likely to display out-group intolerance, immigrant resentment, and support for far-right parties. Per their argument, camp proximity induced "cognitive dissonance [that] led those more directly exposed to Nazi institutions to conform with the belief system of the regime. These attitudes were then transmitted across generations" (573). This argument has profound implications for studying the legacies of genocide, a central research topic in the social sciences (Balcells and Solomon 2020; Charnysh and Finkel 2017; Voigtländer and Voth 2012), and for the historical legacies of past events on current social and political behavior more generally.

As scholars, it is our obligation to learn carefully from the past. In this letter, we show that HPT's results are a product of unobserved spatial heterogeneity. HPT acknowledge that geographical factors may be correlated with survey respondents' proximity to camps. Accordingly, they control for urbanism, being in the former West Germany, and other confounders when estimating the effect of camp proximity on attitudes. Yet, HPT do not account for potential differences in attitudes across Germany's federal states ([*Bundes-*] *Länder*), dismissing Germany's regional heterogeneity as a posttreatment variable (Homola, Pereira, and Tavits 2021). Clarifying the conceptual foundations of posttreatment bias and reviewing the historical record on postwar state creation, we argue that state-level differences confound the relationship between distance to camps and out-group intolerance. When accounting for regional heterogeneity, their findings disappear.

This letter is structured in three parts. First, we situate HPT's argument within the literature on historical persistence and explain why regional differences across Germany may confound their estimates. Recognizing that many scholars are reluctant to condition on variables measured after the causal variable of interest, we use directed acyclic graphs (DAGs) to differentiate between temporal and causal ordering and show the theoretical conditions under which the causal effects of proximity are identifiable given contemporary regional differences. Second, we review the evolution of modern German states, highlighting the postwar *Länder*'s prewar origins. Third, we reanalyze publicly available data employed by HPT (the European Values Survey [EVS] and the 2017 Bundestag elections; see Pepinsky, Goodman, and Ziller 2023A),[1] and show that distance to

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[1] See Appendix A of the Supplementary Material for details on restricted access ALLBUS data, which HPT also analyze.

camps has no explanatory capacity when we account for unobserved state-level determinants of contemporary out-group intolerance.

In addition to showing the fragility of existing results on the long-term effects of genocide, our argument contributes to literatures on spatial econometrics and causal identification in historical persistence research. Like other research, we show that the omission of spatial context can produce incorrect inferences in historical persistence studies (Betz, Cook, and Hollenbach 2018; 2020; Dippel and Leonard 2021; Keele and Titiunik 2015; Kelly 2019). We also clarify the conceptual foundations of posttreatment confounding in historical persistence studies. Here, we join Cunningham (2021), Hünermund and Bareinboim (2021), Pearl (2009), and others in advocating that scholars assess identification conditions formally—either through DAGs or through an equivalent language—rather than appealing to informal arguments about temporal ordering.

## IDENTIFICATION OF LEGACY EFFECTS WITH REGIONAL HETEROGENEITY

HPT argue that geography shapes political attitudes and beliefs and propose that Germans who live near Nazi concentration camps must confront the ideology of the Nazi regime, coping through a form of cognitive dissonance transmitted across generations. The historical legacies literature faces steep inferential challenges and, like most historical persistence research, this argument rests on claims that are impossible to test empirically. For example, the mechanism of cognitive dissonance is not measured directly, but only inferred indirectly. Hoerner, Jaax, and Rodon (2019), who also study the effects of distance to camps on voting for far-right parties, focus on memory satiation as a causal mechanism that explains how cognitive dissonance manifests in electoral outcomes and argue that the effect of distance varies according to the local policy environment. They also include a larger number of German camps in their data than do HPT. Others have noted that camp workers managed the emotional distress of their work using drugs and alcohol (Ohler 2017), a second, alternative coping mechanism to cognitive dissonance. Intergenerational transmission of attitudes toward outgroups is also not measured directly, only inferred indirectly, and given patterns of internal migration and resettlement, it is difficult to estimate the legacy effects of historical variables (Marbach 2021).

Requiring mechanismic evidence places insurmountable obstacles on historical persistence research (Cirone and Pepinsky 2022), but in the absence of evidence of how distance produces out-group intolerance over decade-long time scales, we must consider other possible explanations for this correlation. Variation in political attitudes across Germany's federal states is a well-known feature of contemporary German politics. Given the spatial nature of HPT's argument, we propose a comprehensive strategy to eliminate bias from regional confounders, using regional fixed effects to capture any region-specific features that might confound the effect of historical distance to camps on contemporary out-group intolerance.

German *Länder* differ substantially regarding economic characteristics as well as institutional and policy history, which includes variation in school curricula and civic education (Helbig and Nikolai 2015), meaning that regional differences may directly explain contemporary public opinion. It is reasonable to hypothesize that these differences matter: in HPT's data, the average level of intolerance in Bavaria is half a standard deviation greater than in neighboring Baden-Württemberg (also a Catholic-plurality state in the former West), and the average level of far-right support is more than one standard deviation greater. Factors explaining these differences may include political culture, civil society organization, and political–religious traditions, among others. If these factors are correlated with distance to the nearest Nazi concentration camp, or if the mechanism underlying the effect of spatial proximity on out-group tolerance varies across *Länder* (e.g., due to *Länder* differences in civic education or political culture), then omitting them will generate bias in any estimate of the long-term effect of camp proximity on contemporary attitudes.

If we could measure each and every *Länder*-level determinant of contemporary out-group intolerance, we would be able to isolate the effect of proximity to camps from these competing spatial determinants of intolerance. Practically, this is not feasible with observational data. Fortunately, because those factors are not themselves of theoretical interest but potentially confound the empirical relationship being studied, we can control for them using fixed effects. *Länder* fixed effects adjust for any factor (observable or not) that varies across German *Länder* and explains out-group intolerance, enabling us to estimate the effect of distance to camps on out-group intolerance without measuring those factors directly. Examples of this practice abound in historical persistence research. In addition to Hoerner, Jaax, and Rodon (2019), Charnysh and Finkel (2017) use electoral district fixed effects to study how proximity to Treblinka affects property value and voting patterns. Furthermore, Haffert (2021) employs *Länder* fixed effects to identify the long-term consequences of oppression of German Catholics in the nineteenth century on support for the far-right *Alternative für Deutschland* today. Given that *Länder* fixed effects are standard in historical persistence research, their exclusion requires an explicit justification.

But HPT do not explain their choice to ignore state-level heterogeneity, arguing elsewhere that controlling for contemporary administrative boundaries produces posttreatment bias (Homola, Pereira, and Tavits 2021). Indeed, there are two conditions when one should not control for variables that are correlated with both treatment and outcome:[2] (1) if a confounder lies along the causal path from treatment to outcome, then controlling for it will generate posttreatment bias that

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[2] In this letter, we use the terms *control for*, *condition on*, and *adjust for* interchangeably.

masks the causal relationship of interest and (2) controlling for a variable that is a causal consequence of both the determinants of the causal variable and the outcome generates "M-bias," a form of collider bias. We elucidate these two forms of "bad controls" (Cinelli, Forney, and Pearl 2022, 2) using Pearl's (2009) graphical calculus to characterize how regional fixed effects are used for identification in historical persistence studies.

## Posttreatment Bias

The words "posttreatment," in ordinary language, suggest that any variable measured after the causal variable of interest is a "posttreatment variable" whose inclusion creates bias. This is false. As Cinelli, Forney, and Pearl (2022, 7) observe, "contrary to econometrics folklore, not all 'posttreatment' variables are inherently bad controls." Controlling for posttreatment variables only generates posttreatment bias if they are consequences of the causal variable of interest.

In the language of graphical causal models, a graph encodes causal relationships (represented as directed edges) among variables (represented with nodes). We say that *variable B is a descendant of variable A* if there exists a sequence of edges such that $A \rightarrow \ldots \rightarrow B$ (Pearl 2009, 12–3).[3] Let $F$ denote the confounding variable(s) whose status as a posttreatment variable is in question, let $T$ be the causal variable of interest, let $X$ be a series of observed pretreatment confounders, let $U$ be a series of unobserved pretreatment variables, and let $Y$ be the outcome. The following assumption establishes that controlling for $F$ does not induce posttreatment bias:

> **Assumption 1 (No effect of the treatment on the confounder):** $F$ is not a descendant of $T$.

Assumption 1 shows that the time point at which a variable is measured does not determine whether controlling for that variable creates posttreatment bias. Controlling for $F$ does not create posttreatment bias if Assumption 1 holds: unless distance to concentration camps ($T$) causally affects postwar *Länder* boundaries ($F$), controlling for *Länder* fixed effects cannot create posttreatment bias.

## M-Bias

Even if Assumption 1 holds, controlling for $F$ may still generate collider bias (Pearl 2009, 185–6). A collider is any variable causally influenced by two or more variables; controlling for a collider can generate an association between two variables that are otherwise unrelated (for an overview of collider bias in historical persistence research, see Schneider 2020). Assumption 1 rules out the simplest form of collider bias in which $F$ is a descendant of both $T$ and $Y$. To ensure that

controlling for $F$ does not generate M-bias, we add a second assumption.

> **Assumption 2 (no M-bias):** $F$ is neither a descendant of (a) any variable $U_1$ for which $T$ is also a descendant nor (b) any variable $U_2$ for which $Y$ is also a descendant.

If Assumptions 1 and 2 both hold, then controlling for $F$ generates neither posttreatment bias nor M-bias.

## Posttreatment and M-Bias: Directed Acyclic Graphs

Figure 1 presents three different causal graphs that correspond to three different hypothesized causal structures that are common in historical persistence studies with unobserved regional heterogeneity. These DAGs are arranged temporally, such that variables are realized from left to right, clarifying that $F$ is realized after $T$. To link the DAGs to HPT's analysis, recall that their causal effect of interest is the effect of *Distance to camps* ($T$) on *Intolerance* ($Y$). The issue is whether *Länder* fixed effects ($F$) create posttreatment bias, or whether they help to capture unobserved state-level factors ($U$) that also explain intolerance.

In Figure 1a, both Assumptions 1 and 2 hold: $F$ is not a descendant of $T$, even though $F$ is measured at $t_3$ and $T$ is measured at $t_2$. Controlling for $F$ is essential to estimate the causal effect of $T$ on $Y$ because doing so blocks the backdoor path between $U$ and $Y$. Substantively, if *Distance to camps* does not causally affect the state a respondent lives in, we need to control for state effects to identify the causal effect of *Distance* on *Intolerance*.

In Figure 1b, Assumption 1 does not hold. If $T$ causes $F$, controlling for $F$ induces posttreatment bias. Controlling for $X$ only identifies the causal effect of $T$ on $Y$.

In Figure 1c, the backdoor path from $U$ to $F$ prevents us from estimating the effect of $T$ on $Y$ without controlling for $F$, but controlling for $F$ induces posttreatment bias, so the causal effect of $T$ on $Y$ is not identifiable. Only by observing $U$—which is unobservable by assumption—can we identify the causal effect of $T$ on $Y$. Here, we cannot identify the effect of *Distance* on *Intolerance* unless we can measure every single determinant of intolerance that is correlated with distance to camps.

In Figure 2, we define two unobserved variables, $U_1$ and $U_2$, to illustrate M-bias.

In Figure 2a, Assumption 1 and 2 both hold and the causal path from $U_2$ to $T$ generates a backdoor path that can only be blocked by controlling for $F$. As with Figure 1a, we must control for state effects to identify the causal effect of *Distance* on *Intolerance*. In Figure 2b, by contrast, $U_1$ directly causes $T$ and $F$ and $U_2$ directly causes $Y$ and $F$, so controlling for $F$ will introduce M-bias: we must *not* control for state effects.

In Figure 2c, $F$ directly affects $Y$, rendering the causal effect of $T$ unidentifiable; in Figure 2d, $U_2$ directly affects $T$, rendering the effect of $T$ unidentifiable. To summarize, if Assumption 1 holds but Assumption 2 does not, the effect of $T$ is identifiable if and only if
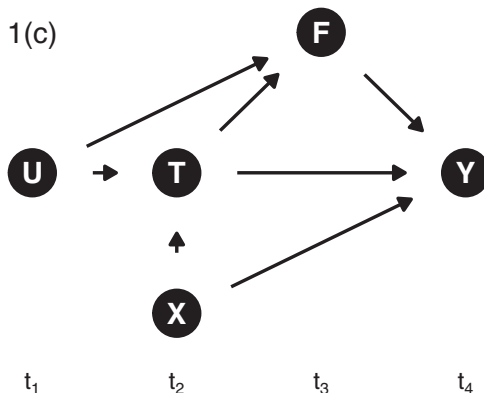
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[3] The ellipsis clarifies that there may be additional intervening nodes along the path connecting $A$ to $B$.

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**FIGURE 1. Directed Acyclic Graphs Illustrating Post-Treatment Bias**
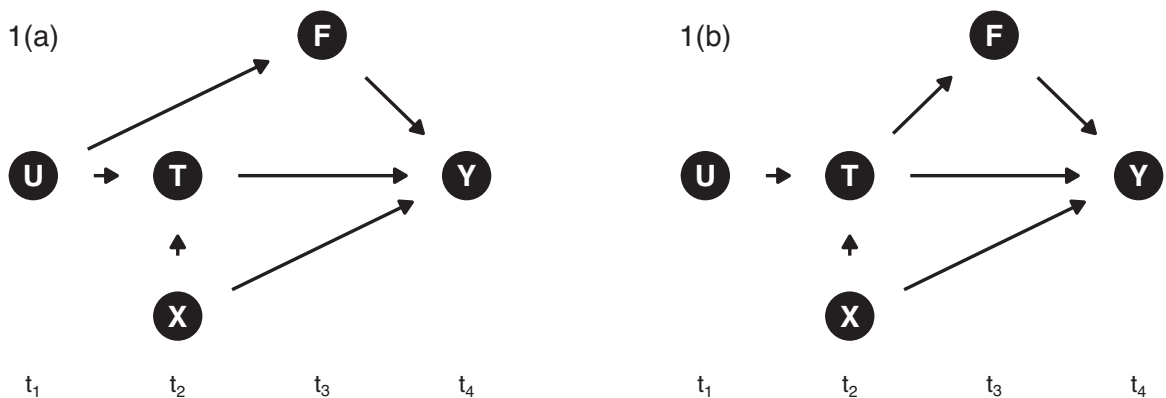
1(a)

Controlling for F and X identifies the effect of T

1(b)

Controlling for X identifies the effect of T,
controlling for F generates post-treatment bias

1(c)

The effect of T is not identifiable
using observed covariates

HPT seek to estimate the effect of Distance to camps (T) on Intolerance (Y).
The issue is whether *Länder* fixed effects (F) create post-treatment bias, or whether they help to capture
unobserved state-level factors (U) that also explain intolerance.

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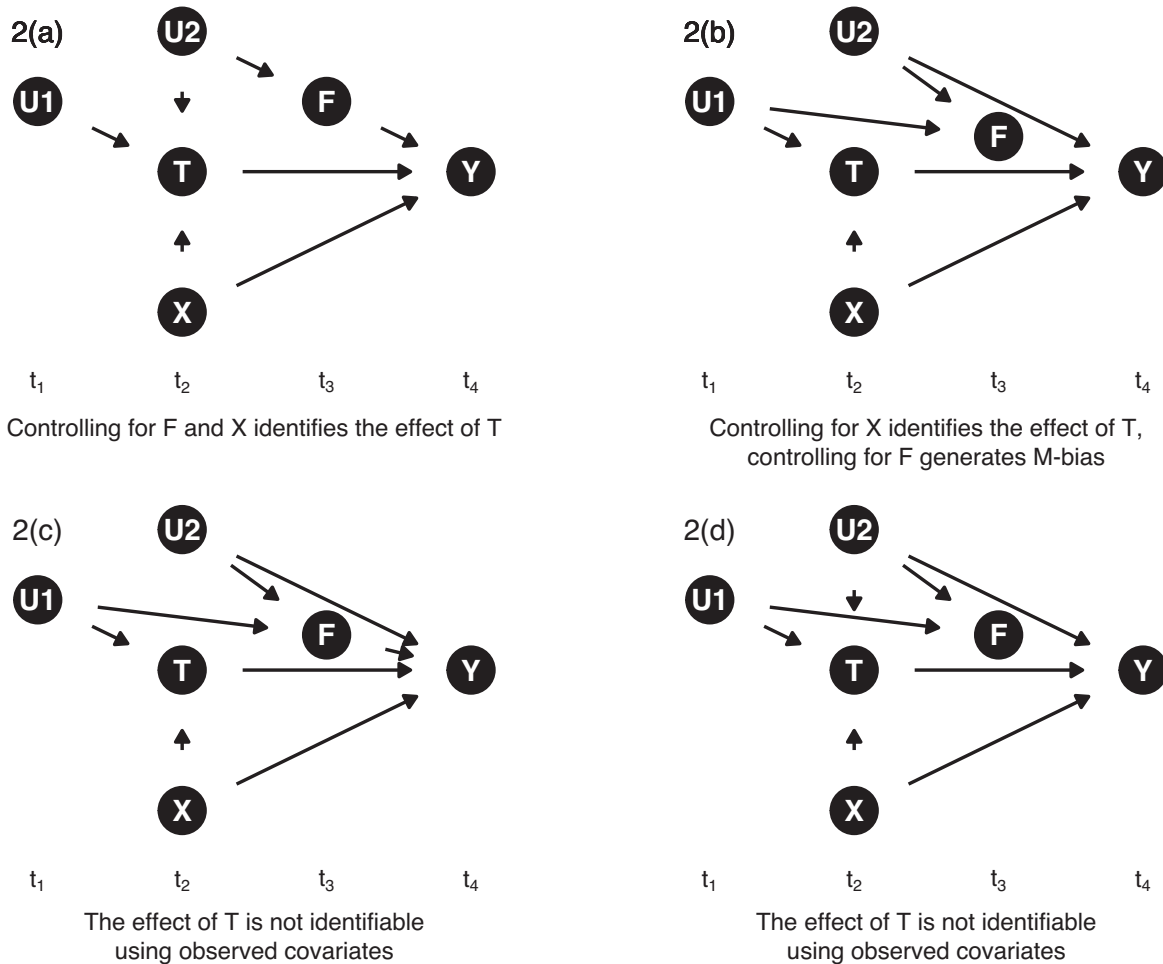$F$ does not affect $Y$ and we observe all joint determinants of $T$ and $Y$.

DAGs encode assumptions about causal structure that are not empirically testable. Faced with uncertainty about the true causal model, then, we rely on substantive knowledge about the causal system under consideration to identify the appropriate model to test the effects of proximity to Nazi concentration camps.

## THE ORIGINS OF *LÄNDER*

Germany's contemporary administrative structure reflects the country's historical political development, but *Länder* cannot be posttreatment variables unless we assume that the creation of *Länder* was caused by their distance from concentration camps. The data do not support this assumption. We draw on qualitative evidence to demonstrate two facts: several postwar *Länder* boundaries predate the Nazi era (in fact, most states of Weimar [1919–1933] were replaced with direct rule and superseded by Nazi *Reichsgaue* [administrative regions]) and postwar state boundaries were not a product of camp location.

Germany's 16 *Länder* have different origins. Bavaria, Bremen, Hamburg, Hesse, Saxony, and Thuringia predate the Nazi era, formed out of previous dutchies and kingdoms. Others correspond to provinces within the Free State of Prussia, the largest state within Weimar: Brandenburg corresponds to the Prussian province of Brandenburg (minus the territory ceded to Poland after 1945), and the Prussian province of Schleswig-Holstein is nearly coterminous with the contemporary state of Schleswig-Holstein. Indeed, Prussian provinces like Schleswig-Holstein were

FIGURE 2. Directed Acyclic Graphs Illustrating M-Bias

2(a)

$t_1$      $t_2$      $t_3$      $t_4$

Controlling for F and X identifies the effect of T

2(b)

$t_1$      $t_2$      $t_3$      $t_4$

Controlling for X identifies the effect of T, controlling for F generates M-bias

2(c)

$t_1$      $t_2$      $t_3$      $t_4$

The effect of T is not identifiable using observed covariates

2(d)

$t_1$      $t_2$      $t_3$      $t_4$

The effect of T is not identifiable using observed covariates

HPT seek to estimate the effect of Distance to camps (T) on Intolerance (Y). The issue is whether *Länder* fixed effects (F) create M-bias, or whether they help to capture unobserved state-level factors (U) that also explain intolerance.

independent political entities with nearly coterminous borders before their incorporation into Prussia. Still other *Länder*, such as North Rhine-Westphalia and Mecklenburg-Vorpommern, were created by occupying Allied powers to reduce the likelihood of renewed Prussian dominance. Baden-Württemberg was formed out of three states straddling different occupation zones in 1952. Saarland was a protectorate under French control until 1957.

To illustrate just how closely contemporary *Länder* align with pre-Nazi administrative units, in Figure 3, we overlay a map of the Weimar borders—drawn in dark gray—with a map of contemporary *Länder*, with concentration camp locations used in HPT's analysis indicated by red dots.

Figure 3 confirms that many contemporary *Länder* boundaries follow borders between states that were established under Weimar or before. Other contemporary borders follow internal borders within Prussia (which contains 7 of the 10 camps in Figure 3), or

borders between occupation zones. These observations support Assumption 1. If distance to concentration camps causally affected *Länder* boundaries, then it should not be possible to identify these boundaries prior to the establishment of the camps. That we can do so helps to rule out the causal diagrams (Figure 1b,c), leaving us with the causal model (Figure 1a) in which *Länder* fixed effects are essential for estimating the causal effect of distance to camps.

A second piece of evidence in support of Assumption 1 is the historical process through which postwar *Länder* boundaries were established. Consistent with our argument, in reviewing the origins of Germany's *Länder*, Gunlicks (2003) explains the emergence of various principalities, states, free cities, and other units that comprised the Second Reich and subsequently the Weimar Republic. From this perspective, the creation of contemporary German *Länder* has largely been determined by historical path dependency and postwar occupation mergers that aligned the new *Länder*

FIGURE 3.   Contemporary German *Länder* and their Weimar-Era Predecessors



geographically with the four occupation zones. We
further review corresponding evidence in Appendix B
of the Supplementary Material.

Assumption 2, which rules out M-bias, is not as easily
subject to empirical scrutiny. We proceed with this
assumption because there is no interpretation of Ger-
man administrative history that matches any of the
hypothetical causal structures in Figure 2.

## REPLICATION

Having explained why *Länder* fixed effects cannot
generate posttreatment bias, highlighting both the pre-
war origins of *Länder* and their postwar creation, we
now show how *Länder* fixed effects affect HPT's
results. Our baseline empirical specification is an exten-
sion of HPT's baseline specification:

$$Y_{is} = \beta Distance_{is} + \gamma X_{is} + \phi_s + \varepsilon_{is} \qquad (1)$$

where $Y_{is}$ is a dependent variable for individual $i$ living in state $s$, $Distance_{is}$ is HPT's measure of the distance to the nearest concentration camp, $X_{is}$ are control variables, $\phi_s$ are *Länder* fixed effects, and $\varepsilon_{is}$ is an error term. $X_{is}$ varies across specifications. In our bivariate model, there are no control variables, an "interwar" specification controls only for HPT's pretreatment confounders,

and a "postwar" specification adjusts for posttreatment confounders following Acharya, Blackwell, and Sen (2016).

## EVS Data

Our results for EVS data appear in Table 1. Each pair of columns compares results with and without fixed effects: in a bivariate model (columns 1 and 2), in HPT's

**TABLE 1. Replication of European Values Study Analysis**

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| **Panel A: Intolerance** | | | | | | |
| Distance to camp | −0.009** | 0.004 | −0.011** | 0.001 | −0.017** | 0.005 |
| | (0.003) | (0.004) | (0.003) | (0.004) | (0.004) | (0.005) |
| % Jews (1925) | | | −1.402 | −2.233 | −1.106 | 8.478 |
| | | | (1.026) | (4.089) | (1.505) | (5.757) |
| % Unemployed (1933) | | | 1.119* | 1.365* | 2.805** | 1.235 |
| | | | (0.481) | (0.618) | (0.833) | (0.921) |
| Population (1925) | | | −0.017* | −0.030* | −0.013 | −0.018 |
| | | | (0.008) | (0.013) | (0.011) | (0.017) |
| Nazi party share (1933) | | | −0.444* | −0.791** | −0.21 | −0.743** |
| | | | (0.182) | (0.227) | (0.226) | (0.251) |
| *Länder* fixed effects | No | Yes | No | Yes | No | Yes |
| Method | OLS | OLS | OLS | OLS | G-est | G-est |
| No. of observations | 2,075 | 2,075 | 2,075 | 2,075 | 1,376 | 1,376 |
| Adjusted $R^2$ | 0.005 | 0.031 | 0.009 | 0.041 | 0.033 | 0.093 |
| **Panel B: Resentment** | | | | | | |
| Distance to camp | −0.106** | −0.029 | −0.116** | −0.041 | −0.106** | −0.018 |
| | (0.016) | (0.025) | (0.017) | (0.025) | (0.021) | (0.028) |
| % Jews (1925) | | | −3.696 | −42.879 | 11.663 | −3.866 |
| | | | (6.104) | (23.733) | (9.418) | (46.528) |
| % Unemployed (1933) | | | 4.161 | 0.607 | 11.930* | 1.913 |
| | | | (2.864) | (3.586) | (5.015) | (5.395) |
| Population (1925) | | | −0.107* | −0.205** | −0.022 | −0.114 |
| | | | (0.046) | (0.077) | (0.067) | (0.101) |
| Nazi party share (1933) | | | −1.728 | −3.515** | −5.164** | −4.947** |
| | | | (1.080) | (1.318) | (1.626) | (1.740) |
| *Länder* fixed effects | No | Yes | No | Yes | No | Yes |
| Method | OLS | OLS | OLS | OLS | G-est | G-est |
| No. of observations | 2,075 | 2,075 | 2,075 | 2,075 | 1,376 | 1,376 |
| Adjusted $R^2$ | 0.021 | 0.09 | 0.022 | 0.099 | 0.058 | 0.15 |
| **Panel C: Far-Right Support** | | | | | | |
| Distance to camp | −0.001* | 0.001 | −0.001* | 0.0004 | −0.003* | −0.001 |
| | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) | (0.001) |
| % Jews (1925) | | | 0.055 | 0.39 | 0.298 | 2.316 |
| | | | (0.230) | (0.928) | (0.461) | (1.218) |
| % Unemployed (1933) | | | 0.064 | −0.055 | 0.368 | 0.21 |
| | | | (0.108) | (0.140) | (0.191) | (0.200) |
| Population (1925) | | | −0.0004 | −0.004 | −0.0005 | −0.006 |
| | | | (0.002) | (0.003) | (0.003) | (0.004) |
| Nazi party share (1933) | | | −0.005 | −0.091 | −0.072 | −0.191** |
| | | | (0.041) | (0.052) | (0.061) | (0.065) |
| *Länder* fixed effects | No | Yes | No | Yes | No | Yes |
| Method | OLS | OLS | OLS | OLS | G-est | G-est |
| No. of observations | 2,075 | 2,075 | 2,075 | 2,075 | 1,376 | 1,376 |
| Adjusted $R^2$ | 0.002 | 0.009 | 0.0004 | 0.009 | 0.019 | 0.026 |

*Note*: Cells contain regression coefficients with standard errors in parentheses. G-est in columns 5 and 6 refers to the sequential *g*-estimator in Acharya, Blackwell, and Sen (2016). Fixed effects coefficients appear in Table A1 in Appendix C of the Supplementary Material. *$p < 0.05$, **$p < 0.01$.

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**TABLE 2.   Replication of Electoral Results**

|  | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| *Panel A: AfD Share* | | | | |
| Distance | −0.055** | 0.036** | 0.072** | 0.068** |
|  | (0.014) | (0.011) | (0.012) | (0.013) |
| Method | Pooled | FE | IWE | RWE |
| FE vs. Pooled | <0.001 | | | |
| No. of observations | 10,870 | 10,870 | 10,869 | 10,869 |
| Adjusted $R^2$ | 0.070 | 0.017 | n/a | n/a |
| *Panel B: AfD and NPD Share* | | | | |
| Distance | −0.067** | 0.038** | 0.071** | 0.066** |
|  | (0.015) | (0.011) | (0.012) | (0.013) |
| Method | Pooled | FE | IWE | RWE |
| FE vs. Pooled | <0.001 | | | |
| No. of observations | 10,870 | 10,870 | 10,869 | 10,869 |
| Adjusted $R^2$ | 0.072 | 0.017 | n/a | n/a |

*Note*: Cells contain regression coefficients with standard errors in parentheses. AfD = Alternative für Deutschland; FE = fixed effects; IWE = interaction weighted; Pooled = ordinary least squares; RWE = regression weighted. FE vs. Pooled displays *p*-values from Hausman tests. All models include prewar covariates; full results appear in Table A4 in Appendix F of the Supplementary Material. *$p < 0.05$, **$p < 0.01$.

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main results (columns 3 and 4), and following their sequential *g*-estimation method (columns 5 and 6).

Across all models, *Länder* fixed effects eliminate the statistically significant negative correlation between distance to camps and out-group intolerance. In five of the nine fixed effects models, the sign flips from negative to positive.

One concern is that fixed effects will mechanically fail to reject null hypotheses when the causal variable of interest varies mainly across rather than within *Länder*. Under our argument that fixed effects are not posttreatment variables, Hausman's (1978) specification test is a formal statistical procedure to evaluate the trade-off between a consistent but potentially inefficient fixed effects estimator and HPT's pooled estimator.[4] In Table A2 in Appendix D of the Supplementary Material, we compare the performance of HPT's pooled specification, our fixed effects specification, and a random effects specification. In every specification, fixed effects models are preferred to pooled models.

In Table A3 in Appendix E of the Supplementary Material, we follow Gibbons, Serrato, and Urbancic (2019), who introduce two reweighting approaches and estimate the effect of *Distance* as a weighted average of state-specific effects of *Distance*, thereby allowing for variation in the effects of *Distance* across states. In Table A4 in Appendix F of the Supplementary Material, we use Weimar-era administrative boundaries to define the regional fixed effects. Our results are unchanged in both analyses.

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[4] If these tests failed to reject the null hypothesis that HPT's pooled models are consistent but more efficient, we would prefer HPT's pooled models to our fixed effects models even though they yield different results for the causal relationship of interest.

### Election Data

Table 2 compares HPT's pooled specification with our fixed effects specifications. To conserve space, we only present models with prewar covariates.

Here, we estimate that the effect of *Distance* on support for far-right parties is positive and statistically significant. Hausman tests reject the null that HPT's pooled models are consistent but more efficient.

### CONCLUSION

The literature on historical persistence in comparative politics must confront serious inferential challenges in studying how spatial effects persist over time. Fortunately, well-understood statistical tools allow us to model unobserved heterogeneity and to assess the plausibility of the assumptions required by statistical models. Using those tools, we have argued that the effect of proximity to Nazi concentration camps cannot be distinguished from unobserved state-level determinants of out-group intolerance in Germany.

Narrowly pitched, these results suggest that the physical remnants of the Holocaust do not play a causal role in shaping contemporary attitudes by writ of their geographical location. More broadly, our results are consistent with an emerging literature in European politics that shows that local-level factors play an important role in shaping out-group attitudes (Hoerner, Jaax, and Rodon 2019; van Heerden and Ruedin 2017; Ziblatt, Hilbig, and Bischof 2019; Ziller and Goodman 2020) while also raising the possibility that attitudinal and material consequences are distinct (Charnysh and Finkel 2017). They also contribute to the established literature on the legacies of regionalism in German politics (e.g., Hepburn

2008). Historical legacies are important, but our findings offer a cautionary tale about how research design matters in identifying how history shapes contemporary politics.

Given the recent growth of historical persistence research in political science (Charnysh, Finkel, and Gehlbach 2023; Cirone and Pepinsky 2022), our argument has implications for empirical practice. A common goal in this research is to estimate unbiased causal effects, which is challenging in the face of observational data with unobserved confounders. Geographic fixed effects are a flexible tool that historical persistence researchers can use to account for unobserved heterogeneity at the level of the geographic unit. Yet critically, we have shown that the decision to use them (or not) requires careful attention to the historical record and to the hypothesized causal structure at hand. We recommend that applied researchers use DAGs to clarify how fixed effects help to identify causal effects by reducing omitted variable bias while ensuring that their inclusion does not create posttreatment bias or M-bias. Careful engagement with the historical record is essential to evaluate the plausibility of the causal structures implied in those DAGs.

Fixed effects are not always appropriate in historical persistence research. Although they readily adjust for unobserved confounders, they cannot be used to adjudicate among different causal factors that are measured at the geographic unit. Fixed effects are also easier to justify in cross-sectional data structures than they are in panel data applications, where temporal feedback processes might violate the assumption of strict exogeneity (Imai and Kim 2019). Even in purely cross-sectional applications, fixed effects are inappropriate if unit boundaries are the direct consequence of the causal variable in question (see Kocher and Monteiro 2016). Moreover, spatial fixed effects do not adjust for confounders that vary within units. We nevertheless conclude that historical persistence research must treat geographic variation carefully, especially when arguments have real-life implications for politics and policymaking.

We conclude with a broader point about studying the Holocaust and the "strength and generalizability of legacy effects" (HPT, 16). This body of research has produced several well-developed theories on coercive institutions and the historical determinants of contemporary attitudes. We hope this enterprise expands to other physical legacies of repression—Confederate-era plantations or mass graves—to test theory, establish scope conditions, and further our collective understanding of history and political violence. We have no doubt that genocide leaves intergenerational legacies, but our argument highlights the extra care needed to measure and model them.

## SUPPLEMENTARY MATERIAL

To view supplementary material for this article, please visit https://doi.org/10.1017/S0003055423000072.

## DATA AVAILABILITY STATEMENT

Research documentation and data that support the findings of this study are openly available in the American Political Science Review Dataverse at https://doi.org/10.7910/DVN/0PY9EA.

## CONFLICT OF INTEREST

The authors declare no ethical issues or conflicts of interest in this research.

## ETHICAL STANDARDS

The authors affirm this research did not involve human subjects.

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